

Ibid. (p. 360).

Microporella Fuegensis, Busk (sp.).

This is not a *Microporella*, as it wants the suboral pore characteristic of this genus. It is furnished with the peristomial pore, which is a leading character of Busk's *Aleonella*; but this has a totally different structural significance.

As there is considerable doubt about the latter genus, I shall postpone the discussion of the systematic place of the present form.

[To be continued.]

XXV.—*A Reply to some Observations on the Mouth-organs of the Diptera.* By B. THOMPSON LOWNE, F.L.S.

MR. CHARLES O. WATERHOUSE in the January number of this Journal appears to invite me to reply to what, for want of a better term, I may designate a "quip courteous," in which he has availed himself of the saving qualities of an "if."

My critic has, curiously enough, seen more in my book than I ever wrote or intended, and has failed to see what I did write; therefore I avail myself of an "if."

If Mr. Waterhouse had used no more acumen in the interpretation of the mouth-parts of the Diptera than he has brought to bear on the interpretation of what I have said I should not have been surprised that he still holds the old and time-honoured opinions regarding the mandible of the dipterous mouth. I do not, however, for a moment suppose that he reads "Nature" as carelessly as he reads my work; but I think he might have rewarded the "skill and care" which he credits me with by a little more attention before he consigned me to oblivion in the pit of error in some unknown region; for if I have fallen into "some error," the nature of which is not even indicated, my position is no better, and there is small chance that a passing friend may draw me out. Therefore it behoves me to make an effort to save myself.

The main argument I use in favour of the views I have adopted is the manner in which the parts in question are developed. If I have falsely interpreted the appearances relating to their development I am as likely to be wrong as another; therefore the question at issue is: Are the mouth-parts of *Musca* developed as Mr. Lowne states or are they not? There are no side issues to the question.

Now with regard to my sins of omission. Mr. Waterhouse is doubtful as to what I mean by parts of the maxilla; yet on page 154 of my book I have indicated that I regard the lancets as homologous with the palpiger and the lacina respectively.

My critic then makes it appear that "I blow hot and cold" according to my necessities. It is true I said that the position of the pseudo-labium is no evidence from a morphological point of view. In this Mr. Waterhouse concurs. But I have nowhere stated that position is never of value in establishing the morphology of a part.

It is possible that two pairs of appendages are fused in the dipterous proboscis; it is conceivable that three are so united, and it is further possible that these limbs are so entangled and crossed, that position would afford no evidence of the morphology of their distal extremities. Moreover the mere similarity of a terminal joint to that of an homologous appendage, such as Mr. Waterhouse appeals to, is not evidence that this joint belongs to a mandible or a maxilla. The hoofs of a horse's feet are similar; but it would be hazardous to conclude that a limb with two hoofs consists of a fore and hind limb united. I see no reason at all why the terminal joint of one of the divisions of the maxilla should not resemble a mandible when it has similar functions to perform.

As regards the simple eye of the flea the case is very different. Its relation to the antenna is such that it cannot be explained on the supposition that the sternal plates of the cephalic segments are dorsal, as the position of the antennæ is explained in *Truxalis* and *Fulgora*. Moreover Mr. Waterhouse supposes an imaginary case, which does not exist so far as we know. It will, I think, be time to consider its bearings on the view I have adopted after its discovery. In the meantime I assume that it does not happen that the compound eye ever bears the relation to the antennæ which the simple eye of the flea exhibits.

Mr. Waterhouse credits me with a consistency which I do not deserve. When I published my book on the Blow-fly in 1870 I never said of the proboscis that "it is mainly formed from the maxillæ." I then regarded it as a complex of several metameres. I was wrong; but until I discovered the manner in which it is developed no one had done so. The contradictory statements of various writers on the subject will themselves speak for the difficulty which exists in making the parts of the proboscis conform to the received theory; and the very fact that some have regarded it as composed of several metameres whilst others deny it metameral characters shows that the theory does not fit with the facts.

Mr. Waterhouse is unfortunate in having ascribed a pre-conceived opinion to me, as my early writings show that I formerly held very different views—views which I have since given up; and I first published the idea that the proboscis is developed mainly from the maxillæ in a short paper in the 'Quekett Club Journal' of 1887.

Although when my present work was first contemplated I was asked to bring out a second edition of my former book, I soon found that it would be entirely new, and Mr. Waterhouse has committed a slight inaccuracy in speaking of my present book as a second edition of one published in 1870. I should have thought that the mere fact of its containing four times the matter already, and a prospect of its containing 700 pages when complete, might have indicated this; moreover it has a different title. But perhaps it is too much to expect in these days of high pressure that those who quote a work should look at the title.

XXVI.—*Description of a new Species of Sminthus from Kashmir.* By OLDFIELD THOMAS.

THE remarkable genus *Sminthus*, which, *nota bene*, is a member of the Dipodidæ*, not of the Muridæ, has been until recently considered to consist of only a single species, *S. subtilis*, Pall. (*S. vagus*, auct. plurim.), ranging from Denmark to Central Asia. Quite recently Dr. Büchner † has described a second species, *S. concolor*, from Ganssu, China, a species which has not the characteristic black dorsal stripe of *S. subtilis*; and I have now the opportunity of describing a third one of the same most interesting group.

The type specimen is a skin with skull, obtained in Kashmir, at an altitude of 10,000 feet, by Major G. H. Leatham, of the East Surrey Regiment, in whose honour I propose to name the species

Sminthus Leathemi, sp. n.

Extremely similar in size, colour, and general appearance to *Mus sylvaticus*, from a British specimen of which it is at first sight hardly distinguishable, except that the ears are decidedly smaller.

* This view of the true affinities of *Sminthus*, first published by Winge in 1887 ('Gnavere fra Lagoa Santa—E Museo Lundii,' p. 109), had been held by me long previously, and I still think it is unquestionably correct.

† Bull. Ac. Sci. St. Pétersb. xiii. p. 267 (1892).